Authors' reply to Referee 1 comments on the TCD manuscript "Assessment of permafrost distribution maps in the Hindu Kush Himalayan region using rock glaciers mapped in Google Earth" by M. O. Schmid et al.

We would like to thank the referee for his constructive comments, which helped to improve this paper.

Referee comments are in bold, author reply's without formatting and *changes to the manuscript in italic.* The feedback of the Referees had two important points in common that we address here:

A) The relation between rock glaciers and permafrost

The initial manuscript may have been misleading in a way that Referees questioned whether rock glaciers really delineated the lower limits of permafrost existence, when in fact, we purposefully avoided the term and concept of permafrost limits. Our understanding is that rock glaciers are not suitable to delineate the boundaries of permafrost, as ground thermal conditions are spatially too heterogeneous to justify the concept of limits. Extensive research has shown, however, that rock glaciers frequently occur near the lowermost regional occurrence of permafrost in mountains. The manuscript reads now as follows:

The occurrence of rock glaciers is governed by the ground thermal regime and by the availability of subsurface ice derived from snow avalanches, glaciers, or ice formation within the ground. Furthermore, sufficient supply of debris as well as topography steep enough to promote significant movement is required. As intact rock glaciers contain ice (latent heat) and move downslope, their termini can be surrounded by permafrost-free ground. The frequently occurring cover of coarse clasts promotes relatively low ground temperatures and thereby further retards the melting of the ice within the rock glacier. This makes termini of rock glaciers local-scale indications for the presence of permafrost, frequently occurring at an elevation indicative of the lowermost regional occurrence of permafrost in mountains (Haeberli et al., 2006). This tendency of begin among the lowermost occurrences of permafrost in an area is exploited in this mapping exercise. The spatially heterogeneous ground thermal regime and the frequent existence of permafrost-free areas directly adjacent to rock glaciers makes the concept of "permafrost limits" impractical as these limits are neither measureable nor clearly defined and consequently we avoid this concept despite its prevalence in the literature. In more gentle terrain, such as parts of the Tibetan Plateau, not

the ground thermal conditions (i.e. the presence of permafrost), but the slope angle is the limiting factor. Therefore, the presence of rock glaciers can be used as an indicator of permafrost occurrence, but the absence of rock glaciers does not indicate the absence of permafrost. Mapped rock glaciers will thus result in a conservative estimate of the actual permafrost distribution, as over large areas of permafrost no rock glaciers can be present due to the lack of debris, low slope angles, lack of avalanche snow or the elevation of the valley floor.

B) Difficulties to understand to concept of a mapped candidate area (Fig. 6, 7 and 8)

The rock glacier mapping in our study is only meaningful for areas where rock glaciers can potentially exist. There are most likely vast regions in the HKH region, mainly on the Tibetan Plateau, where rock glaciers are absent due to the lack of topography and debris. For those we cannot perform an assessment of the available permafrost distribution maps. To exclude such areas we created the concept of the mapped candidate area, which includes only the area where we can potentially expect the presence of rock glaciers. This reduced investigation area does not include all mapped samples anymore, but only the sample areas which fulfil certain criteria concerning topography, satellite image quality and glacier coverage. This mapped candidate area is then the basis for the assessment of the available permafrost distribution maps. The manuscript reads now as follows:

Rock glaciers outside the signatures for permafrost provided by the evaluated maps indicate false negatives, as the map indicates the likely absence of permafrost, but the existence of permafrost was inferred based on mapped rock glaciers. A comparison of mapped rock glaciers with predicted permafrost extent, however, is only informative in situations where the formation and observation of rock glaciers can be expected. *In the further analysis we excluded all parts of the initial samples where no rock glaciers can be expected. This subset of our mapping was named potential candidate area and includes only sample areas, which fulfil the following three criteria*: (a) Topography: Only sample polygons where the vertical standard deviation of the SRTM 90m DEM is larger than 85 m. This threshold was chosen so as to be smaller than the lowest observed value where rock glaciers were mapped, which is 89.5 m. (b) Image quality: Only samples with sufficient image quality in Google Earth were taken into account. (c) Absence of glaciers: Glacier covered areas were excluded based on the glacier inventory published by Bajracharya and Shrestha (2011), which largely covers the HKH region with the exception of parts of China.

I endorse the arguments for getting a better handle on the distribution of permafrost in High Asia, and much more attention to the largely neglected topic of rock glaciers there, for this and other purposes. Great concentrations do occur in certain mountain

ranges, and offer a way to appreciate the occurrence and complexities of cryosphere conditions and a basis for tracking changes.

I also agree that this task is severely constrained both by the sheer extent, diversity and logistical difficulties of the terrain and environments of interest, and the near total lack of any concerted research in most of the high mountains, least of all into rock glaciers. As such there is an urgent need to exploit the high resolution satellite imagery that has become available, and I agree that it now shows good diagnostic detail for identifying RGs, their dimensions and diversity of forms, and sub-regional differences.

As a contribution to Cryosphere Discussion, important questions arise as to:

i) how far and how well prevailing notions of rock glaciers and permafrost, largely developed elsewhere, apply in poorly or un-researched areas of High Asia;

ii) how far rock glaciers relate to permafrost, are sensitive or effective indicators of its extent or boundaries; and

iii) the promise and reliability of emerging GIS methods in a vast, complex, and data-poor region.

Regrettably I find the paper as developed so far, hard to follow. The methodology and statement of results seem unconvincing. A much better appreciation of the nature of rock glaciers is required, their relations to permafrost, and implications of what is seen in the HKH.

MAJOR CONCERNS

The basic hypothesis or purposes of the study seems to be: use of rock glaciers (RGs) as indicative of permafrost, especially its lower elevation limits in the HKH region, and as a test and extension of two existing permafrost maps. In principle this seems fine, but:

1. First, the results cast doubt on the purpose and conclusions. It is stated that "Comparison of the two rock glacier mappings showed relatively small differences indicating that the proposed mapping procedure works consistently."(p.5306 l.7-8) However, apparently the "mappings" only identify or are reliable *in 4%* of the 4.5 million km2 region of interest! They exhibit a larger area (26%) with uncertainty, and exclude over 70% of the region. Does this not suggest that either the hypothesis, or the method used to test it, are, at best, inefficient or marginal to the problem?

AC: In total we mapped 4000 sample polygons, each with an area of approximately 30km^2. In 4% of all samples both mappings contained rock glaciers and in 93% of all samples neither mapping contained rock glaciers. To us this shows, that rock glaciers are relatively rare in the investigation area, but does not say anything about the reliance or the efficiency of the chosen method. In fact this is what could be expected because of the definition of the HKH region by ICIMOD which contains large parts outside the high mountains. We believe this comment to be based on a misunderstanding of our strategy and findings and have reformulated the corresponding results section: "*Of the 4,000 samples 3,432 (86%) received the same classification by both mapping persons: 70% did not have any rock glaciers, 12% had insufficient quality and 4% contained rock glaciers (Fig 3). In 3% of all samples only one mapping contained rock glaciers but the other did not.*" (New Manuscript l. 243)

For similar reasons the results say very little about the two region-wide permafrost maps. The "first order" (?) differences or agreements seem sketchy for IPA, and very local and marginal for PZI (?)

AC: See comment above.

The results in Fig. 6 and 8, give an impression of complex and fine-tuned findings, but it is not clear to this reviewer what they mean. How does Fig 6. reveal RGs "…in relation to Permafrost Zonation Index summarized *over the entire HKH region*" -- <u>if only established for 4% of it???</u>

AC: Regarding a mapping of only 4%, this is due to rock glaciers being relatively rare when looking at the entire region. The legend now reads: "….. *over the mapped HKH region*" to be more conservative in our claims. (New Manuscript l. 533)

And in areas I know I cannot make sense of Fig. 8. It shows yellow squares in the core of the NW Himalaya/Karakoram/Hindu Raj area suggesting " there is only permafrost in favorable conditions". Surely, there is only ever permafrost under favorable conditions! However, in these sub-regions there are not only large areas of permafrost, but also hundreds if not thousands of RGs.

AC: This figure is indeed conceptually difficult and we have improved our explanation of this analysis in several parts. The legend here, however, clearly states what the colours refer to "Spatial patterns of agreement between mapped rock glaciers and PZI. Colour indicates the lowest PZI value in the mapped rock glaciers within each 1° x 1° square. Green and yellow are signalling an apparent good agreement between lowest elevations reached by rock glaciers and predicted lowest possible elevations for permafrost by the PZI." To make this point even more clear, we have now also modified the figure and its legend.

Meanwhile:

2. A more critical assessment is needed of why the authors are convinced that RGs in these high mountain environments are, or can be, used delineate the limits of permafrost. My work suggests considerable caution on this. Even in the Alps and subpolar regions there has been a progressive retreat from the early view that permafrost is a prime factor in the origin of RGs, let alone definitive of them. Certainly, in the Hindu Kush, Hindu Raj, Karakoram, and NW Himalayan Ranges, with which I have some familiarity:

i) a majority of RGs depend primarily on avalanched snow and rockfall or talus deposits, on glaciers up above or transitional to RGs, typically some combination of all these. They drive the development, scope, downslope reach and fluctuations in RGs, but relations to permafrost are unkown. At least, an explanation is needed for assuming that lowest or 'mean minimum lowest' reach would depend upon, or reflect the presence of, permafrost, rather than the scale and strength of avalanching, rockfall, glacier and wind driven processes.

ii) Experience suggests that, in addition to RGs "… which do not reach the regional lowermost occurrence of permafrost.,,(p, 5307, l.12) there are many others that reach below it.

iii) A key determinant of the lowest reach in any given valley and, presumably, mean minimum elevation of RGs (not permafrost), *is the elevation of valley floors*. This is determined by landscape and stream system evolution, in which permafrost is a dependent variable too. Thus, the RGs you reference in England and Owen (1998), descend as low as 4,300 m (there are many others in the same valleys terminating higher and up to 4,900 m). However, within 50 km to the west and north, many RGs descend below 3,900 m and some down to 3,400 m. There is no reason to think RG-generative conditions are much different, but valley floors are incised lower.

AC: Please see our general answer for the relation rock glaciers and permafrost

3. I have trouble with various aspects of the statistical procedures and results.

Is 'random sampling' as used here, an appropriate method? It is one thing to select at random to prevent bias in sampling for characteristics distributed within a known population. But thousands of random spatial samples in order to find some particular item in this vast region seems like searching for needles in haystacks? Moreover, it must provide randomized outcomes based not on your concerns, but on probability

distributions of regional terrain. It seems unlikely to be good at discriminating the comparatively rare RGs.

AC: We decided to use a random sampling strategy because we do know so little about the rock glacier distribution in the HKH. This implied that we would have many samples without rock glaciers (needle in the haystack), but still we ended up with 155 samples containing more than 700 rock glaciers (p. 5305 l. 5). Therefore random sampling seems to be a feasible approach to map rock glaciers in the HKH region. We agree with this reviewer that there may have been a more effective way to generate this data, but had we chosen that route, then we might have to justify later why we made certain assumptions during our sampling. Our results as they are presented are not affected by this choice.

Incidentally, we *know* there are tens of thousands of individual RGs clustered across the whole region! In this sense I am surprised that all your results involve only 'one, two, or occasionally 'more than three" RGs. In hundreds of valleys in the NW Himalayan ranges and, no doubt, other parts, there are concentrations of dozens of RGs within radii of 10-30 km.

AC: In Figure 5 we made those three classes because if there are only one or two rock glaciers in the sample polygon, results have to be treated slightly more cautious, than if there are many more rock glaciers. In fact in 58% of the samples containing rock glaciers there were three or more rock glaciers. Also there are 21 samples with ten or more rock glaciers and a maximum of 21 rock glaciers in two samples.

The caption for Figure 5 reads now as the following: "Mean minimum elevation of rock glaciers per sample. The size of the square indicates on how many rock glaciers this value is based on. *This is for 24% one rock glacier, for 18% two rock glaciers and for 58% between three and 21 rock glaciers."* (New Manuscript l. 533)

If we scale up our results (our random samples represent about 2.5% of the entire area) then 700 rock glaciers scale to 28,000 over the entire area. These are only the ones mapped by both operators. Assuming that some features are hard or impossible to distinguish on images or may be counted as separate lobes when seen in the field, it is plausible to assume in excess of 100,000 rock glaciers in this area, fully in line with the proposition of this reviewer.

"Mean minimum elevations per sample" (Fig 5 etc)? Not sure what this implies. You seem to have a lot of cases with only one or two RGs per sample, making a mean minimum value seem meaningless? (eg. in Fig. 5). Conversely, how is it valid to compare such with others having three or more. Again, this disregards readily

available evidence that, in valleys with numbers of RG's, termini elevations typically vary and may range over 100's, if not a thousand metres, when permafrost does not?

AC: We have chosen a mean value over an absolute minimum value, as it is more robust against potentially misinterpreted landforms (p. 5304 l. 4). We think it does give an appropriate indicator about permafrost conditions for a specific sample, where ground surface temperature, and thus permafrost, may vary considerably on even very small scales (Gubler et al., 2011). We share your concern about the comparability of values derived from differing amounts of RGs per sample and for that reason, Figure 5, already in the original manuscript provides a visual representation of the amount of rock glaciers mapped per sample. The caption for Figure 5 has been adjusted and now adds more detail: "Mean minimum elevation of rock glaciers per sample. The size of the square indicates how many rock glaciers this value is based on. *This is for 24% one rock glacier, for 18% two rock glaciers and for 58% between three and 21 rock glaciers."* For the relation between rock glacier and permafrost please see our general answer

I am surprised just two operators are seen as sufficient to establish or preclude operator error in such a complex task and visual procedures -- even assuming you could get started without some common set of instructions and discussion with them, which is bound to affect selection procedures and make it entirely possible both would be wrong while producing identical results (?) With respect to operator error, the lowest elevation lines at RG snouts appear the critical ones and from what you show they seem to differ little. However, this begs two questions;

i) does one *or either* trace show the actual lower limit of the active RG. You appear to assume it does, but I am not at all sure. The images in my copy are not of the best resolution, but Figs 2.and 4 are good enough to raise doubts about how much of what you show *inside* each operator's trace, can be confidently treated as active RG. They look suspiciously like examples I know that combine active, inactive and 'fossil' areas, while margins in this steep terrain may involve debris derived from RG activity, but not part of the active body.

AC: The rock glacier mapping was conducted by three operators (p. 5301 l.24), this resulted in two comprehensive mappings (p. 5302 l. 1). For the analysis we only used areas delineated in both mappings as rock glaciers (p. 5302 l. 1). Even after two independent mapping of each rock glacier we can not give a guarantee that every point within the delineated areas is part of an intact rock glacier. Still we are confident that in the majority of the cases the mapping is correct and even more though for the rock glacier snouts. To

increase transparency and make results more reliable we attached both mappings as supplements to our manuscript.

ii) If RG termini are spread over a range of elevations, it is unclear to me how taking a mean value for two or three, or even ten or fifty, gets any closer to the lowest elevation of permafrost, being at most, a very crude value of where permafrost may occur

AC: Please see the answer to your comment above

iii) In such an exercise, the complete lack of any ground control is problematic, or any indication of attempts at field checks or experience with RGs anywhere. Nearly all our knowledge of rock glaciers and related permafrost issues is based on field studies, and translating from them to remotely sensed data needs to be spelled out.

AC: We agree with the referee that direct measurements of permafrost (boreholes) or indirect measurement (geophysics / seismology) to complement our results would be very beneficial and desirable. For the huge area we covered this is not really a valid option and we therefore decided to rely purely on satellite images. Rock glaciers have previously been mapped based on remote sensing images around the world (Janke, 2001, Brenning, 2005, Fukui et al., 2007b, Lilleøren and Etzelmüller, 2011, Lilleøren et al., 2013) (p. 5299 l. 10ff), but, to our knowledge, never using only Google Earth.

MINOR MATTERS

p. 5298 line 23-4? "Many of the investigated rock glaciers have developed out of Little Ice Age moraines…" Isn't this based on assumption? Of the tiny number of RGs investigated in the HKH, are there *any* actual age determinations or established histories, let alone "many'? Also, views of the LIA, its duration, intensities and uniformity or otherwise across High Asia, are all being contested; also whether Eurocentric views haven't misled us as to what has happened there.

AC: Agreed, sentence removed.

p.5298 l.22-3 Hewitt (2014) is cited but evidently not consulted. Nowhere does he state or imply there are "lowermost elevations… around 4,000 m". The tables and surveyed examples in his Chapter 11 include RA termini at 3,500 m and some down to 3,350m (this in the W. Karakoram, which might have led to a comment on the "lowest elevation" you cite, in Northern Afghanistan of "3,554 m"). He also reports a nearly 1750 m difference between lowermost termini across the Greater Karakoram region surveyed (his p.275).

AC: The statement was corrected accordingly to Table 11.1 in Hewitt (2014) and reads now as the following: "*For the northern regions of India and Pakistan, in the Karakorum Range, lowermost elevations of active rock glaciers vary between 3,850 and 5,100 m a.s.l. Inactive rock glaciers were even recorded at lower elevations with a minimum elevation of 3,350 m a.s.l. in the Western Karakorum Range (Hewitt, 2014)."* (New Manuscript l. 145)

The lowest elevation of 3,554 m a.s.l. is based on our mapping and not a citation, nevertheless it is in agreement with Hewitt (2014)

p.5303, l.10 "…transversal and longitudinal flow structures, providing a subjectively acceptable, but here not objectively testable, level of confidence in interpreting landforms as intact." Does "intact" mean 'Active'? If so, this is not reliable, 'subjectively' or otherwise. In HKH 'ridge-and furrow' "flow structures" can be highly developed and may persist indefinitely in inactive features, even in relict RGs.

AC: Intact relates to rock glaciers which contain permafrost. To visually define the ground thermal conditions of permafrost related landforms is difficult, for both remote sensed based mappings and actual field mappings. To overcome this issue we mapped every scene two times independently taking into account flow structures (longitudinal and latitudinal), frontal appearance and outline visualization. The reformulated manuscript reads now as the following: "It was possible to assess *visually* the steepness or activity of the rock glacier front and the characteristic of transversal and longitudinal flow structures, providing a subjectively acceptable, but here not objectively testable, level of confidence in interpreting landforms as indicators for the presence of permafrost." (New Manuscript l. 256)

Also, I suggest a further caution concerning; "…Vegetation coverage, an indicator of inactive or relict rock glaciers…"

Apart from the roles of lithology, elevation and local climate, there is extensive, intensively practiced mountain pastoralism almost throughout HKH areas where your RGs occur. Active RGs are avoided, but inactive and relict RGs can be heavily used, and modified by grazing, firewood collection and temporary summer residences. Also, vegetation cover is not everywhere a reliable indicator of 'inactive' RGs. In some areas I have observed *active* ones with a ground cover.

AC: Yes, we agree and have formulated that now more clearly: "*Vegetation coverage on a rock glacier was only identified in two sample polygons in the whole HKH region and is either absent in the investigation area, or not visible based on the imagery available. In European mountains, vegetation cover has often been taken as an indication of relict rock glaciers (Cannone and Gerdol, 2003) but this concept is difficult to generalize to other mountain*

ranges. The two cases mapped here have been disregarded for further analysis." (New Manuscript l. 260). We have not discussed this in much detail before as only two cases were observed.

Your descriptions are highly interesting and we would be interested to know how you have assessed the inactive rock glaciers to still contain ice and how heavy their vegetation cover was.

p.5304 l.10 "If variations within close proximity occur, they follow regional patterns." In such a vast region and complex task, you need to specify just what the 'variations', and 'close proximity' mean here, and which "regional patterns" are followed?

AC: We forgot to refer to Fig 5 here, which we've corrected now. It is a description of what can be seen in Fig 5 and should be clear to the reader when looking at the figure.

p.5306 "A clear increase in the minimum elevation reached by rock glaciers can be observed between the south and the north side of *the mountain range*." The HKH region as shown in Fig.1 has many huge mountain ranges. Are you saying that in all of these you expect RGs to descend lower on northerly than southerly? Can the very limited and scattered identifications really support this conclusion? In my experience other factors reverse this relation in some areas, as they do for glaciers and snowlines.

AC: North and South did not refer to aspect, but to the position in our investigation area. To make matters more clear we changed the sentences to: "A clear increase in the minimum elevation reached by rock glaciers can be observed *towards the Tibetan Plateau."* (New Manuscript l. 354)

References

Bajracharya, S. and Shrestha, B.: The status of glaciers in the Hindu Kush-Himalayan region., ICIMOD, Kathmandu., 2011.

Brenning, A.: Geomorphological, hydrological and climatic significance of rock glaciers in the Andes of Central Chile (33-35°S), Permafr. Periglac. Process., 16(3), 231–240, doi:10.1002/ppp.528, 2005.

Cannone, N. and Gerdol, R.: Vegetation as an Ecological Indicator of Surface Instability in Rock Glaciers, Arctic, Antarct. Alp. Res., 35(3), 384–390, doi:10.1657/1523-0430(2003)035[0384:VAAEIO]2.0.CO;2, 2003.

Fukui, K., Fujii, Y., Mikhailov, N., Ostanin, O. and Iwahana, G.: The lower limit of mountain permafrost in the Russian Altai Mountains, Permafr. Periglac. Process., 18(2), 129–136, doi:10.1002/ppp.585, 2007.

Gubler, S., Fiddes, J., Keller, M. and Gruber, S.: Scale-dependent measurement and analysis of ground surface temperature variability in alpine terrain, Cryosph., 5(2), 431–443, doi:10.5194/tc-5-431-2011, 2011.

Haeberli, W., Hallet, B., Arenson, L., Elconin, R., Humlum, O. and Ka, A.: Permafrost Creep and Rock Glacier Dynamics, Permafr. Periglac. Process., 17, 189–214, doi:10.1002/ppp, 2006.

Hewitt, K.: Glaciers of the Karakoram Himalaya, Springer Netherlands, Dordrecht., 2014.

Janke, J. R.: Rock Glacier Mapping: A Method Utilizing Enhanced TM Data and GIS Modeling Techniques, Geocarto Int., 16(3), 5–15, doi:10.1080/10106040108542199, 2001.

Lilleøren, K. S. and Etzelmüller, B.: A regional inventory of rock glaciers and ice-cored moraines in norway, Geogr. Ann. Ser. A, Phys. Geogr., 93(3), 175–191, doi:10.1111/j.1468-0459.2011.00430.x, 2011.

Lilleøren, K. S., Etzelmüller, B., Gärtner-Roer, I., Kääb, A., Westermann, S. and Guðmundsson, Á.: The Distribution, Thermal Characteristics and Dynamics of Permafrost in Tröllaskagi, Northern Iceland, as Inferred from the Distribution of Rock Glaciers and Ice-Cored Moraines, Permafr. Periglac. Process., 24(4), 322–335, doi:10.1002/ppp.1792, 2013.